

the 12th to the 25th, when removed by rain and warm spell. The moisture soaked in, improving the soil. Early fall sown wheat was in thrifty condition, but the late sown was retarded by dry soil and lack of rain and was not so vigorous.—*G. N. Salisbury.*

West Virginia.—The weather was generally quite cold during the month, and there was considerable snowfall. Wheat and rye were generally well protected, but the prospects were poor. Stock was wintering well, with prospect of sufficient feed. No plowing was done.—*E. C. Vose.*

Wisconsin.—The month as a whole was decidedly cold, the average temperature for the State being but 0.3° above the average for January, 1904, which ranks among the coldest Januaries during the past thirty-four

years. The snowfall for the State averaged about thirteen inches and was fairly well distributed. Winter grains and grasses were thoroughly protected during the month by an ample covering of snow.—*W. M. Wilson.*

Wyoming.—A cold wave overspread the State on the 11th, 12th, and 13th, but as it was not accompanied by much snow, stock did not suffer any injury. Another storm and cold wave was quite general over the southern half of the State at the close of the month, and some apprehension was felt in regard to stock. As a whole, the month was favorable for stock, which remained in good condition, with practically no losses reported.—*W. S. Palmer.*

SPECIAL ARTICLES.

ESCAPE OF GASES FROM THE ATMOSPHERE.

By DR. G. JOHNSTONE STONEY, F. R. S.

[Reprinted from London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science, June, 1904, 6th series, vol. 7, p. 620.]

A letter under the above heading by Mr. S. R. Cook, in Nature of the 24th of March, 1904,¹ puts forward views that ought not to remain on record without reply; and as between thirty and forty years ago I carried on the investigation into the rate at which gases can escape from atmospheres in the same way as Mr. Cook has done, and arrived, from the premises employed by him, at substantially the same conclusions, perhaps the best answer will be to state the considerations which led me to distrust that line of argument and finally to abandon it. To do this, however, requires more to be said than can be brought within the compass of a letter to a weekly journal; and on this account, and because the discussion is a physical discussion and concerns one of nature's greater operations, I venture to request for the following pages the hospitality of the Philosophical Magazine.

A study of the phenomena attending the escape of gases from atmospheres has been approached in two ways—*inductively*,² by arguing upward from events that are found to have occurred or to be in process of occurring in nature; and *deductively*,³ by drawing inferences from the supposition that it is legitimate to attribute to the real gases of nature behavior which it has been ascertained would prevail in certain models of gas, so much simpler in their constitution than real gases that the progress of events within them is susceptible of mathematical treatment.

The two methods, as hitherto employed, have led to contradictory results, of which one, at least, must be erroneous. Mr. Cook, who has of recent years employed the deductive method, expresses the opinion in his letter that the numerical results which have been arrived at by this method "will have to stand" until they can be disproved "by other *a priori* reasoning".

Serious students of nature must, I think, hold that man, in his dealings with nature, is not in position to limit in this way the kind of proof he will accept, and that it is sufficient if in any way Mr. Cook's inferences from Maxwell's researches can be disproved, whether by valid *a priori* or by valid *a posteriori* reasoning. And, moreover, that when once they are disproved we are brought face to face with the fact that there has been a mistake somewhere in the data which have led those who trusted in them to a false conclusion.

¹ In the Monthly Weather Review for August, 1902, p. 401, we also have published a very suggestive paper on the above subject by S. R. Cook. But it deals with problems on the very boundary of the present state of our knowledge, and when learned authorities differ we must in all honesty present both sides of the case to our readers. We accordingly reprint Doctor Stoney's conservative conclusions, as showing the need of further investigation before the subject can be considered definitely settled.—*C. A.*

² "Of atmospheres upon planets and satellites." By G. Johnstone Stoney, F. R. S. See Scientific Transactions of the Royal Dublin Society, vol. 6, p. 305, October, 1897; or Astrophysical Journal, January, 1898, vol. 7, p. 25.

³ "On the escape of gases from planetary atmospheres according to the kinetic theory." By S. R. Cook. See Astrophysical Journal, January, 1900, vol. 11, No. 1.

What convinced me several decades ago that the conclusion at which I arrived and at which Mr. Cook has arrived is false, is that it represents the moon as incompetent to get rid of the atmosphere which it originally shared with the earth, and of the gases which it has since evolved in abundance from its own interior. We knew thirty-five years ago, as we know now, that any reasoning which makes out that the moon has retained its atmosphere must have a flaw in it somewhere. Furthermore, since that time other facts not then known have come to light and in a marked degree confirm the judgment which was then formed. Our confidence that we are on the right track is justifiably strengthened when, as in this case, further discoveries as they emerge confirm the view to which we had been led when our materials were more scanty. The presence of helium on the earth was not then known, and the argument⁴ which has been based on what is now known of its behavior may be summarized as follows: helium is supplied to the earth's atmosphere through certain hot springs, and under circumstances which indicate that it also oozes up through the soil. It is, however, what is carried up by the water of these springs that can be subjected to experimental examination. The other gases of our atmosphere, such as nitrogen, oxygen, and argon are found to accompany the helium in these springs, but with this marked difference, that whereas the other gases are present in such proportions as are consistent with their merely being portions of those gases which are being returned to the atmosphere after having been washed down into the earth from the atmosphere by rain, the case is entirely different when we come to helium. The quantity of helium passed into the atmosphere through those springs is found to be from 3000 to 6000 times more than can be accounted for as a return to the atmosphere of helium which had been washed down out of it. Accordingly we are justified in regarding this great surplus of helium as being an addition which is being uninterruptedly made to the atmosphere. Notwithstanding this, the quantity of helium in the atmosphere has not gone on increasing. The earth at the present rate of supply furnished in a small number of years a quantity of helium equal to the quantity which the atmosphere can at present retain, i. e., in a number of years which is exceedingly small from a geological standpoint, which is the point of view that is here appropriate. The inference from these facts is the obvious one, that helium is by some agency being eliminated from our atmosphere as fast as it is being introduced into the atmosphere from the earth. Two possible agencies for the elimination of the helium suggest themselves, chemical reactions and an escape of helium from the upper part of the atmosphere. Of these, chemical agency is excluded by the extreme chemical inertness of helium. What remains then is that there is an outflow of helium from the top of the atmosphere equal to the inflow at the bottom, and that the trace of helium that is at any one time present in the atmosphere is helium part of which is slowly making its way upward to the

⁴ The argument here summarized is based on the marvelous determinations made by Sir William Ramsey, K. C. B., F. R. S., or in his laboratory, and will be found with the necessary details in a paper on the behavior of helium in the earth's atmosphere. By G. Johnstone Stoney, Astrophysical Journal, vol. 11, p. 369, 1900.

situation from which some of its molecules can escape, and so produce that outflow which balances the net influx at the bottom of the atmosphere.

Having satisfied myself that the deductive method as I applied it, and as Mr. Cook has applied it, lands us in erroneous results, I set to work to scrutinize the data of the deductive argument with a view to ascertaining how far they may be depended upon and at what points they are doubtful. All branches of physics require us to be more or less on our guard against trusting without sufficient scrutiny to inferences from that mixture of theory and hypothesis of which we are obliged to make use in order to be able to employ mathematics in physical research. The demand for this caution becomes a pressing one when, as in gases, we are obliged to deal with immense numbers of events, each of which has its own dynamical history with incidents peculiar to itself, and where what chances on some of these occasions differs enormously from that which occurs in most of them. Of this kind are the interactions between the molecules of a gas and the inter-fused aether, and especially those complicated struggles between molecules which we call their encounters, events each of which, when viewed as it ought to be viewed, from the molecular standpoint, is a battle lasting a long time, as time has to be measured in molecular physics, and with an immense number and variety of incidents. These, the interactions between the molecules and aether, and the interactions between molecule and molecule, are the primary events, the real determining events, which occur within a gas; while the movements of the molecules as they dart about between one encounter and the next, the spectrum radiated by the gas, the ions which present themselves after some of the encounters, the compounds which result from chemical reactions during some of the encounters (if what we are dealing with happens to be a mixture of suitable gases), and, finally, that remarkable partition of energy between the events going on within the molecules and the translational motions of the molecules, which is effected during some of the encounters—all of these are subordinate events depending upon those which are above spoken of as the primary events. When dealing with such almost immeasurably intricate and obscure operations of nature, it behooves us with the very utmost caution to distinguish between what is theory and what is hypothesis in the data we employ, in order to be able to ascertain how far any conclusions we draw follow from the one, and how far they involve the other, with the risks inseparable from it.

Theories are suppositions we hope to be true; hypotheses are suppositions we expect to be useful. As to theories, they are either correct or erroneous. They may be, they usually are, but they by no means need to be, of use to man. The virtue of a theory is simply to be true. On the other hand hypotheses usually make use of machinery which we can see to be simpler than that operating in nature; and especially is this the case with the hypotheses to which we are obliged to have recourse in mathematical investigations, which, in order to be of use, must be so great a simplification of the complex intricacies of nature that human mathematics shall be able to cope with them.

The theory of gas universally put forward in scientific books when the present writer was young was the erroneous statical theory that the molecules of a gas may be stationary, that they have a capacity for expanding and contracting, and that each molecule presses against its neighbors. An illustration frequently made use of in those days was that of a froth of bubbles pressing against one another. This erroneous theory had the field in Avogadro's time, and for more than thirty years afterwards; but in the fifties of the nineteenth century it was gradually, though not without protest, displaced (chiefly through a masterly series of papers by Clausius) by the kinetic theory, which is now the prevalent theory. The kinetic theory

of gas, as formulated by Clausius, regards the molecules of a gas as missiles of equal mass, darting about in space and not acting *sensibly* on one another except when "encounters" chance to take place, i. e., not until the centers of mass of two molecules get within an interval of one another, which is less—usually much less—than the average length of the free paths which the molecules describe between the encounters; which free paths are accordingly approximately straight and pursued with unvarying speed, except so far as they may be slightly influenced by gravity or other external cause, or by some excessively minute part of the interactions between molecules, if any such survives when the intervals between molecules get beyond what we may call their encountering distance.

This is the kinetic theory of gas as put forward by its founder,⁵ and any system of bodies which conforms to this definition may be called a *kinetic system*. Thus, there are in nature as many kinetic systems as there are distinct gases; and moreover all those models of gas in which the progress of events has been studied by mathematicians *are each of them* a kinetic system. So also are the cosmic bodies of celestial space, if we eliminate from the definition the condition that the masses must be equal; and, in fact, some modification of this clause of the definition is essential, even as regards gases, inasmuch as in all gases of nature there are found some of the missiles differing in mass from others: thus, in diatomic gases ions present themselves with masses that seem to be half the mass of the average molecules.

We may add further details without trespassing beyond the domain of theory, i. e., while still endeavoring to describe events as they occur in nature. Thus, we may add that elaborate internal events are going on within all these missiles, which internal events absorb about one-third of the whole available energy of the gas; and we know that two partitions of energy take place—one a partition of energy (which probably goes on uninterruptedly) between these internal events of the molecules and the events of the aether, the other a partition of energy which now and then occurs with comparative suddenness between the internal events of the molecules and their translational motions. This latter transfer of energy seems to take place only when two molecules are in grip with each other during an encounter, and not at every encounter, but only during those which take place under certain necessary conditions. If, as seems probable, encounters with these special characteristics are as rare as those which result in the breaking down of molecules into ions, or of those which result in chemical reaction in a mixture of equal volumes of chlorine and hydrogen, then the infrequency of their recurrence can be estimated; and, in cases in which it has been found possible to make the estimate, the infrequency seems to range from one out of 10^9 encounters down to one in 10^{15} , when we pursue the observations so far as they have been recorded.

It is here that I strongly suspect, though I am not in a position to claim that I know, that the mistake has been made by Mr. Cook and by my friend Professor Bryan, who both tacitly assume that this partition of energy is a process which goes on uninterruptedly, even in the upper parts of the atmosphere. Whether the mistake be here or elsewhere, as yet may be only highly probable; but that a mistake exists somewhere in the premises of the deductive argument was placed beyond question by nature when she presented to us events that have occurred or are occurring, which negative some of the inferences to which those data lead. We may be unable with certitude

⁵ Clausius's papers were preceded by a paper by Waterston, which was presented to the Royal Society in 1845, but which was not then published. This paper, when it long afterwards came to be printed, was found to contain a most valuable anticipation of the kinetic theory as developed by Clausius. If Waterston's paper had been printed in due course the kinetic theory would probably have been adequately dealt with some years sooner.

to put our finger upon the precise spot where the mistake came in, but that mistake has come in somewhere can be proved.

When Maxwell determines his law for the distribution of speeds in a kinetic system, he exercises a caution⁶ which has not always been observed by his successors, and is careful to present the law as the law governing the distribution of speeds (not in every, or indeed in any gas), but in a kinetic system which consists of numberless equal particles, each of which is a perfectly rigid and perfectly elastic sphere, after an immense number of collisions have taken place—assumptions which he afterwards varied in different ways, as by substituting particles of other forms, or points repelling one another inversely as the fifth power of the distance. The several assumptions which he thus makes are put forward not as theory but as hypothesis; they do not profess to reproduce any existing gas, but substitute for the gas an artificial model; and Maxwell is careful to keep this prominently before the mind of his reader.

As to his exponential law for the distribution of speeds, it is the solution of a functional equation, which in turn is the expression of the assumption that the number of molecules whose velocities lie between u , v , w , and $u + \delta u$, $v + \delta v$, $w + \delta w$ must be some function of u , v , and w . Now this is true of Maxwell's models, but can not be the case in any gas in which there is an irruption of energy from the internal motions to the translational on the occurrence of events which depend either wholly or partly on conditions other than the mere translational speeds of the molecules—such conditions, for example, as the aspects of the two molecules to one another when the encounter is about to take place, or the phases at which the internal motions had arrived at that instant of time, or many other conditions that are possible and can be easily conceived. Accordingly, whenever a mathematician applies Maxwell's law under the impression that, as regard any particular gas, it is more than an approximate law, he tacitly assumes either that there are no internal events (as in Maxwell's models), or that if there be internal events, as in all real gases, the partition of energy between these internal events and the translational motions is a transfer taking place at such short intervals that it may legitimately be treated by the mathematician as a process which goes on continuously and at a constant rate. At the bottom of our atmosphere an event that happens once in 10^9 encounters occurs to each molecule as often as seven or eight times per second. Even here the assumption that the transfer of energy goes on uninterruptedly makes but a rough approximation to the truth, and it is utterly remote from being an approximation in that penultimate stratum of the atmosphere from which nearly the whole escape of molecules takes place, and especially in regard to an event like the escape of a molecule from the earth, which is mainly the outcome of the circumstance that an individual encounter has chanced to be very unlike the ordinary encounters. Hence, in no real gas can the actual law of distribution of speeds be identical with Maxwell's exponential law, nor with any of the exponential laws of Maxwell's successors; although under the conditions which prevail in our laboratories these laws may be an approximation sufficient for many useful purposes.

The cases in which Maxwell's approximate law may legitimately be employed can be pointed out. Whenever an approximate law presents itself in an exponential form with a negative index, the approximation holds good as an approximation over that small part of the range where the exponential function acquires large values, but can no longer be depended upon as an approximation in regard to the parts of the range where the exponential function is small. Maxwell makes a legitimate use of his law when, through its instrumentality, he discovered his remarkable explanation of viscosity and diffusion, and investigated the laws of these phenomena. In reference to

these, what happens in the case of velocities which are infrequent is of small account; but the application made by Professor Bryan and Mr. Cook is to the rare events which occur within that part of the range where the approximation breaks down, and where, in consequence, the exponential law is misleading. It is to this oversight to which I think it likely that we are mainly to refer numerical results which are found to clash with events that have taken place or that are taking place upon the moon and on the earth.

The inquiry in which I engaged in the sixties of the last century led also to the detection of other defects in the premises made use of by those who have trusted in the deductive method. One of these concerns the ambiguities which surround the use of the term "temperature". Temperature is not one physical measurement, but two groups of physical measurements, essentially different according as we test equality of temperature by there being no transfer of heat by conduction when two bodies are brought into contact, or by radiation when they are made to stand apart. This establishes a division of temperature into two principal groups, and these groups require further subdivision.

The temperature of a body determined in these two different ways may be called its conduction temperature, of each of which there are several varieties. There are accordingly many different kinds of temperature. In the case of gases, conduction, (including convection), is mainly concerned with the translational speeds of the molecules, while radiation in the first instance affects only the internal events going on within the molecules. In most laboratory experiments (carried on as they must be at the bottom of our atmosphere) the partition of energy between the internal events of each molecule and its transitional movements takes place so frequently—probably several times every second in a gas at standard temperature and pressure—that the distinction between the two main kinds of temperature does not need to be attended to. But, to go to the opposite extreme, let us consider the case of a gaseous molecule which has escaped from the earth and travels like an independent planet through space. Here no interchange of energy can take place between the translational movement of the molecule and its internal events. Under suitable external influences either of them may be made to vary to any extent without this affecting the other. The two kinds of energy, or, if we please to call them, of temperatures, have become divorced; and intermediate stages between these extremes would be found to exist within an atmosphere if we could explore it from its bottom to its top.

Further distinctions have to be made within the two principal kinds of temperature. Those which have to be taken into account in the present investigation are the varieties of radiation temperature. A body, like the sun, acting by radiation upon different gases has no one definite radiation temperature, but may be at a different radiation temperature in regard to each gas. Thus, the sun is hotter with regard to the helium of the earth's temperature than with regard to its hydrogen. This we know, because the radiations of the sun which can affect hydrogen come in the form of the rays corresponding to the hydrogen lines of the solar spectrum which are dark, while the radiations which raise the temperature of helium come through rays corresponding to the helium lines, of which the principal one within the visible spectrum—the double line D_3 —is as bright as the neighboring part of the spectrum. Hence, the radiation which reaches helium in the outer part of our atmosphere has the full intensity of radiation from the sun's photosphere.

Reviewing the whole case, we find that in the stratum of the earth's atmosphere from which helium escapes, the opportunities of exchanging energy between the internal motions and the translational, instead of occurring to each molecule several times per second, may be so infrequent that they occur only

⁶ See Maxwell's Scientific papers, vol. 1, p. 380; or Philosophical Magazine, January, 1860.

once in several hours. During all its intermediate flights the molecule is exposed during the daytime to the full glare of radiation as intense as direct radiation from the sun's photosphere. In this way the internal motions of the molecule will be kept for some hours excited to intense activity, and if during these hours that special kind of encounter happens to take place which affords an opportunity for an interchange between the internal and translational energies, the two encountering molecules will fling asunder with what may be described as explosive violence. All that is then necessary for a molecule to escape is that one of the two that have encountered shall have the direction of its flight outward, that it shall have sufficient speed, and that it shall escape other encounters. If the chance that these events shall happen befalls each molecule in the penultimate stratum of the helium atmosphere as often as once in several days, there would probably be an abundant outflow of helium from the earth to account for the observed rate of its escape.

Here, however, we are on debatable ground. We can only follow events in detail with probability, not with certainty. But on the other hand, when we trust to the inductive argument based on the ascertained behavior of helium, as stated in an earlier paragraph, *we are on secure ground*. We may rely on the conclusion to which it leads, viz: that helium is escaping from the earth's atmosphere, and that the rate of escape is the same as the rate of the net inflow from the earth into the atmosphere. By the net inflow is meant the supply after deducting something like $1/6000$ or $1/3000$ part of the whole, in order to allow for the very minute quantity of helium that had been washed out of the atmosphere by rain and which is being restored to it.

There are other matters, too, which would need to be understood and allowed for before we should be entitled to trust the deductive method of proof. Thus, the internal events that go on within the molecules of matter are of more than one kind, and in gases stand differently related to the translational motion. This is revealed to us by phosphorescence and other phenomena. An attempt to make a preliminary classification of these internal events has been made by the present writer in a memoir on the kinetic theory of gas.⁷ But without going into these and other matters, enough has been said to show how inadequate the deductive method is—at least as hitherto handled—to be a safe guide in dealing with the matters with which it has been made to grapple. This, of course, also shows that objections based on investigations of this character have no weight against the testimony about the rate at which gases do actually escape from atmospheres which is given by such *facts* as the absence of atmosphere from the moon and the behavior of helium upon the earth.

The objection urged by Mr. Cook against accepting the inductive proof of the actual rate of escape of gases from atmospheres is analogous to the objection urged by some scientific men when in 1867 I brought forward a proof⁸ that in an atmosphere of mixed gases the atmosphere of each gas must have a different limit, the lighter constituents overlapping and extending beyond those that are denser. "Oh," it was then said "that can't be the case. It is inconsistent with Dalton's law of the equal diffusion of gases". Yet I have lived to see my conclusion generally, I believe universally, accepted by physical astronomers; and I look forward with some hope to an ultimate acquiescence in what is now being objected to in reference to escape of gases from atmospheres. In both cases the objection rests on the same error—the mistake of hypoth-

esis for theory, and the consequent mistake of a law which is approximate for a law of nature.

THE COORDINATES OF THE UNITED STATES WEATHER BUREAU STATION AT MOUNT WEATHER, VA.

By HERBERT HARVEY KIMBALL, Librarian and Climatologist.

This station is located on the summit of the Blue Ridge Mountains, in Loudoun County, Va. As determined from the Harpers Ferry contour sheet of the United States Geological Survey, its latitude is $39^{\circ} 4'$ north, its longitude $77^{\circ} 53'$ west from Greenwich. The location and surroundings of the station are shown on fig. 1.

No precise leveling has been done in this locality by either of the Government surveys. The Southern Railway has determined grades and elevations on its branch line from Alexandria to Bluemont, Va., the latter point being only about six miles from the Mount Weather station. Unfortunately, the profile constructed from the railway surveys is in two sections. The first extends from Alexandria to Round Hill, Va., the original terminus of the road; the second is the extension from Round Hill to Bluemont. The point of connection between the two sections is not clearly defined, and for this reason doubt was entertained as to the accuracy of the elevation of Bluemont as determined from these profiles.

The nearest Government survey bench mark is at Point of Rocks, Md., about 30 miles from Mount Weather, and the Chief of the Weather Bureau therefore instructed me to run a line of levels from this bench mark to Mount Weather. That part of the survey between Bluemont and Mount Weather was made in August, 1904, the remainder in November following. With the exception of about twelve miles of railroad between Bluemont and Paeonian Springs, Va., most of the route followed the country roads, on which at many points the grade was exceedingly steep.

Starting from the top of the upper end of a railroad culvert just east of the station at Bluemont, the summit of the Blue Ridge was reached by way of Snickers Gap, and the county road near the summit followed to the Mount Weather station. Here the outer corner of the top of the north foundation pier of the water tower was selected as a bench mark.

As a check upon this part of the work, which was the most difficult of all, and also to determine heights in the valley immediately below the Weather Bureau station, the survey was extended down the side of the Blue Ridge to Trapp, Va., and then back to the starting point at Bluemont by way of the Loudoun Valley. The difference in elevation between the culvert at Bluemont and the bench mark at Mount Weather was found to be 1019.903 feet by way of the mountain road and 1019.981 feet by way of Trapp and the valley road, a difference of only 0.078 of a foot. This is considered very satisfactory in view of the fact that on the mountain it was impracticable to make backsights and foresights equal in length on account of the steep grade, the many short turns in the road, and the obstruction of the view by trees.

From the railroad culvert at Bluemont to Paeonian Springs, Va., the survey was along the track of the Southern Railway, and foresights and backsights were made equal in length by counting the ties between stations. At Paeonian Springs we left the railroad and followed the highway to Point of Rocks, Md., by way of Waterford and Taylorstown, Va., crossing the Catoclin Mountains after leaving Taylorstown. The foresights and backsights were kept as nearly equal in length as was possible from eye estimates of distance.

There were few opportunities to check the accuracy of this part of the survey. The exact location of stations occupied by the railroad engineers could be determined in only a few cases. My determination of the height of a nearly level piece of track just west of Hamilton, Va., is 1.4 feet higher than the

⁷ "Of the kinetic theory of gas as illustrating nature". By G. Johnstone Stoney, F. R. S. Scientific Proceedings of the Royal Dublin Society, June, 1895, vol. 8, p. 356; or Philosophical Magazine, October, 1895, p. 362.

⁸ "On the physical constitution of the sun and stars". By G. Johnstone Stoney, F. R. S. Proceedings of the Royal Society, No. 105, p. 1, 1898. See, especially, paragraphs 23, 24, 25.